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# Compensating the Cooperators: Is Sorting in the Prisoner's Dilemma Possible?

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Choice between different versions of a game may provide a means of sorting, allowing players with different preferences to self-select into groups of similar types. We experimentally investigate whether auctioning off the right to play a prisoner's dilemma game in which the cost of unilateral cooperation is lower than in the status quo version separates (conditional) cooperators from money maximizers. After the auction, significantly more subjects cooperate in the modified PD than in the status quo PD whereas there is no difference between cooperation rates if the two versions of the game were assigned to participants. However, sorting is incomplete and cooperation deteriorates over time. The auction price does not correspond to the differences in expected values between the modified and the status quo game.

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## I Introduction

Experiments on prisoner's dilemma and other public goods games typically reveal cooperation rates higher than the equilibrium prediction in the first round and a decrease over time, leading to cooperation rates closer to the equilibrium prediction in the final round of the game. This pattern applies to repeated games as well as to repeated one-shot games.[1] In this paper, we investigate whether auctioning off the right to play a prisoner's dilemma game may stop this trend. The paper explores whether the choice between two versions of a prisoner's dilemma game with the same Nash equilibrium but different out-of-equilibrium payoffs provides a means of sorting, allowing players with different preferences to self-select into the version of the game they prefer.

The existing experimental findings on prisoner's dilemma and public goods games overwhelmingly suggest the existence of heterogeneous players, the two most important being conditional cooperators (i.e. subjects who cooperate if others cooperate as well) and egoists (i.e. money maximizers).[2] Most recently, Fischbacher et al. (2000) showed the relevance of conditional cooperation in a public goods game, finding that the majority of their subjects were conditionally cooperative (with a strong correlation between own and other contributions).[3] However, despite the prevalence of conditional cooperators, high cooperation rates cannot be sustained over time. As conditional cooperators' behavior depends on what others do, the existence of non-cooperative types induces a downward spiral.[4]

We investigate whether an auction for the right to play a modified version of the game rather than the status quo version separates cooperators from defectors. In the modified game the cost of unilateral cooperation is lower, but the Nash equilibrium remains the same as in the status quo game. We choose this specific change in out-of-equilibrium

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[1] See, for example, Andreoni (1988), Andreoni and Miller (1993), and for surveys, Davis and Holt (1993) and Ledyard (1995).

[2] See, for example, Brandts and Schram (forthcoming), Croson (1999), Keser and van Winden (forthcoming), and for a recent survey, Fehr and Gächter (2000). Individual heterogeneity in preferences does not exclude the possibility of errors. Rather, studies testing the relevance of errors and of other-regarding preferences in public goods games find that both are present, see Anderson et al. (1998), Andreoni (1995) and Palfrey and Prisbrey (1997).

[3] 48 percent of the subjects were conditionally cooperative and 32 percent could be classified as purely selfish. The remaining 20 percent of the subjects displayed an unusual, not easily identifiable pattern of behavior.

[4] Fehr and Schmidt (1999) show theoretically that cooperation cannot be obtained in equilibrium even if the

payoffs as not only the theory but also earlier experimental evidence (e.g. Ahn et al. 1998) suggest that when games are assigned, such a change in payoffs does not affect behavior. In addition, it captures important aspects of real life mechanisms employed to sort employees, customers and insurees. As the cost of unilateral cooperation is lower in the modified version than in the status quo version, the change in the payoff structure represents an insurance mechanism by (partially) compensating the cooperator in case his or her counterpart defects. Therefore, the modified game is prima facie more attractive to players who want to cooperate than to money maximizers. Apart from sorting schemes used by insurance companies for example, clubs often employ similar mechanisms to induce self-selection. E.g., high membership rates together with certain privileges can deter some people and attract others who value these privileges highly enough.

The experiment is designed as follows: We run two versions of a one-shot two-person prisoner's dilemma game with the same unique Nash equilibrium. Before playing the game, each subject individually decides which version of the game he or she wants to play. The right to participate in the "insured" instead of the status quo version of the game is sold in an $n^{th}$-price auction. The subjects who win the auction play the game according to the modified payoff structure while for all others the status quo version remains valid.

We test for the effect of two central contextual variables, the number of rights available to play the modified PD and the number of periods played after an auction. The intuition for this is straightforward: If there are more rights available than conditional cooperators present, full separation of player types is impossible. Even if all cooperators opted for the modified game, some egoists would be able to take advantage of them, inducing the downward spiral. If there are fewer rights available than conditional cooperators present, on the other hand, sustainable sorting seems possible. We vary the number of periods played after an auction to test for different expected values of playing the insured version of the game. The more periods subjects can spend in the "safe(r) haven", the higher their bids should be.

This is a novel experimental design. In contrast to earlier related experimental studies, our design investigates the choice between two versions of a game instead of between playing and not playing a game. The latter choice situation has been extensively studied for

conditional cooperators are in the majority.

bargaining and coordination games.[5] The exit experiment by Orbell and Dawes (1993) comes closest to our design. In their experiment, subjects could choose between playing a prisoner's dilemma game (where a player's profits were only positive if the other player cooperated, otherwise he or she made a loss) and exiting the game (with payoff zero). The authors report higher cooperation rates with an exit option than in the standard PD and argue that this supports sorting: Egoists opt out as they underestimate the probability of cooperation while cooperators choose to play the PD.

In our experiment, assuming type-contingent beliefs is not necessary to account for sorting. Rather, rational expectations can induce players to select the version of the game they prefer. Also, our design allows for a better test of the sorting hypothesis because we observe the behavior of those who lost the auction, whereas in the design of Orbell and Dawes (1993) those who exit have no choice to make. We find that in the first period of the auction treatments significantly more subjects cooperate in the insured game than in the status quo version. Paying for the right to participate in the insured game seems to provide an opportunity for self-selection. However, sorting is incomplete. In most sessions there is some cooperation in the status quo game and some defection in the insured game. The auction price does not correspond to the differences in expected values between the insured and the status quo game and the price predicted in a sorting equilibrium is not reached in the laboratory.

While the laboratory environment seems comparatively simple, the bidding decision is cognitively quite demanding. In order to bid rationally, subjects would have to hold correct beliefs about the cooperation rates in both games. With two player types, conditional cooperators and money maximizers, this requires knowledge of the distribution of types in both games after the auction. We find that while the first-period cooperation rate is significantly higher in the insured version than in the status quo version of the game, there are still many cooperating subjects who are "exploited". The experience of being the "sucker" induces the disappointed cooperators to stop cooperating.[6] We find that

[5] In bargaining games, proposer competition was analyzed by Güth and Tietz (1986) and responder competition by Prasnikar and Roth (1992). Van Huyck et al. (1993) and Cachon and Camerer (1996) allow players in a coordination game with Pareto-ranked equilibria to opt out of the game. Cooper et al. (1993) investigate the effect of an outside option in the battle-of-the-sexes game. Related to these exit-experiments is a public goods game by Erhart and Keser (1999) in which subjects could form new groups.

[6] For early experimental evidence in psychology, see Brubaker (1975). For more recent economic

cooperating subjects whose counterpart defected in the first period are equally as unlikely to ever cooperate again as subjects who defected in the first period. Only after mutual cooperation are first-period cooperators willing to cooperate again in future rounds. This dynamic leads to a decrease in auction prices and cooperation rates in the insured game over time.

In the next part II of our paper, we outline the experimental design. In section III, we derive predictions. Section IV reports the experimental results and Section V discusses their relevance. Section VI concludes the paper.

## II Experimental Design

Our design consists of a two-person, one-shot prisoner's dilemma game, which is employed in two versions. Table 1 presents payoff table A, the status quo version, and payoff table B, the insured version of the game. Numbers represent actual payoffs in cents. Defection is the unique Nash equilibrium in both versions, which only differ in the out-of-equilibrium payoffs for unilateral cooperation. The payoffs were presented to our experimental subjects in a matrix form; no normative frames were used.[7]

Table 1:

Payoff Table A

| | X | Y |
|---|---|---|
| X | 350 ; 350 | 0 ; 500 |
| Y | 500 ; 0 | 150 ; 150 |

Payoff Table B

| | X | Y |
|---|---|---|
| X | 350 ; 350 | 100 ; 500 |
| Y | 500 ; 100 | 150 ; 150 |

experiments, Isaac et al. (1989).

[7] For the experimental instructions, see appendix A.

The game was repeated five times, which was common knowledge. Subjects were randomly matched with a new counterpart in each period[8] and privately informed about their individual results after each period. Four different treatments were conducted: The control treatment I where games A and B were assigned to the participants, as well as three different auction treatments. In the latter, we varied the number of periods played after an auction as well as the rights available to play game B. Our choice of the number of rights available to play game B was influenced by prior results on the number of cooperators present in prisoner's dilemma games. Reviewing the literature revealed a striking consistency: Typically, cooperation rates of about one third are found in the first rounds of one-shot prisoner's dilemma games.[9] We label treatments with more rights to play game B available than expected cooperators as "large groups B" and those with about equally as many rights available as "small groups B".

In treatment conditions II and III, auctions were repeated in every period, in condition II with large B-groups (approx. two thirds of the participants) and in condition III with small B-groups (approx. one third of the participants). We chose the group sizes so that in condition II, B would consist of more players than the number of cooperators we expected, and in condition III, B would be small enough to consist of cooperators only. In treatment IV, also a small-group design (approx. one third of the participants), an auction was only run in the first period, after which subjects remained in the respective games for periods 2 to 5 (subjects were randomly re-matched in each period as above). An overview of the experimental design is presented in table 2.

[8] See Andreoni and Miller (1993) for this repeated one-shot design in two-person prisoner's dilemma games.
[9] See for one of the earliest prisoner's dilemma experiments, Dawes et al. (1977) who report a cooperation rate of 33 percent in an 8-person PD and, most closely related to our design, Andreoni and Miller (1993), who find a cooperation rate of about 35 percent in the first period. Cooperation rates depend on the social distance between the subjects and between the subjects and the experimenter. We guaranteed complete anonymity similar to a double-blind design (see Bohnet and Frey 1999a and b).

Table 2: Experimental design

| **Treatment conditions** | **N** | **n(A)** | **n(B)** |
|---|---|---|---|
| I. Control | 48 | | |
| Assigned A | | 24 | |
| Assigned B | | | 24 |
| II. Repeated auction, B large | 72 | | |
| Auction 1 | 26 | 10 | 16 |
| Auction 2 | 24 | 10 | 14 |
| Auction 3 | 22 | 8 | 14 |
| III. Repeated auction , B small | 78 | | |
| Auction 4 | 30 | 20 | 10 |
| Auction 5 | 18 | 12 | 6 |
| Auction 6 | 30 | 20 | 10 |
| IV. First round auction, B small | 54 | | |
| Auction 7 | 28 | 18 | 10 |
| Auction 8 | 26 | 16 | 10 |

An $n^{th}$-price sealed bid auction was used to elicit individuals' willingness to pay for game B. Even though experimental evidence does not fully support the theoretical predictions (Vickrey 1961), comparatively stable behavior is reported in private and common value second-price auctions (Kagel 1995). However, the auction employed here neither qualifies as a private value nor as a common value auction. Our bidders do not know the value of the item to themselves with certainty as the value depends on other participants' choices in the game. Allowing for subject heterogeneity, the value of the item is also not the same to everyone: One's own preferences and expectations about other people's behavior determine bidding. Van Huyck et al. (1993), who used an auction to sell the right to play a game for the first time (eliciting individuals' willingness to pay for a coordination game), call it a "game form auction". They state that "the value of the object being auctioned is determined by the strategic interaction of the owners and this strategic interaction can depend on the price generated in the auction." (Van Huyck et al. 1993: 493).

Auction sessions were conducted as follows: After the participants had read the instructions and questions had been answered, we ran a practice auction where subjects were assigned their true private valuation for a hypothetical good beforehand.[10] We then started with the experiment: Each participant had to indicate his or her bid; the auction price was determined and written on the blackboard. Subjects were allocated to game A or game B depending on their bid, randomly paired with another player in the same version of the game and informed about which game they were playing. Then each subject had to decide whether to cooperate or to defect; finally everybody learned about his or her earnings in the current period. In treatment conditions II and III, this procedure was repeated five times; in treatment condition IV, the auction was only conducted in period 1 and players remained in either game A or B for all five periods.

The experiments were conducted with students from various universities in the Boston area. Participants were paid a show-up fee of $5. The experiment was conducted anonymously and took approximately 45 minutes. After the experiment, participants were paid in cash and earned $15 on average (including the show-up fee). Subjects were identified by code numbers only and care was taken that neither other subjects nor the experimenter could observe private decisions.

## III Predictions

We derive our reference prediction from standard game theory. If all subjects are rational money maximizers, and if this is common knowledge, the equilibrium prediction is identical in all our treatment conditions: Nobody cooperates in either version A or version B of our game, and nobody is willing to bid anything for the right to play version B rather than version A in the auction treatments.[11] However, as discussed above, there is much empirical evidence for standard prisoner's dilemma games, such as the one employed in our control treatment, suggesting that about one third of the players cooperate in the first

[10] Participants were asked to indicate their willingness to pay for one of three identical hypothetical goods on a piece of paper, which we collected. We wrote all the bids on the blackboard and demonstrated how a second price auction works: The three highest bidders would each win one of the identical hypothetical goods and pay the price that the fourth highest bidder bid. In case of a tie, a random device was used to determine who won the auction, and the price announced before the tie occurred had to be paid.

[11] Due to backward induction, this prediction follows even if we acknowledge that random matching does not

period of one-shot public goods games with binary choices.

Taking these findings into account, the standard game theoretic prediction formulated above is modified. We propose a simple but straightforward definition of types: As money maximizing rational individuals should not cooperate in one-shot prisoner's dilemma games, we consider all players (and only those) who cooperate in the first period to be conditional cooperators. Both, conditional cooperators and egoists, are willing to pay a positive price to participate in version B rather than version A of the PD, given that they do not assume that only subjects of their own type are present[12] and given that egoists do not assume that the likelihood of being paired with a cooperator is the same in both versions of the game (in which case they bid zero cents). If subjects expect that there are both money maximizers and conditional cooperators in the group, sorting is possible.[13]

Assuming a constant proportion of cooperators of one third, we make a point prediction and a comparative statics prediction for first-period behavior:

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produce a "true" one-shot environment.

[12] If this is common knowledge, everybody bids zero cents and egoists defect and cooperators cooperate in both games. This is an extreme version of the "false consensus effect" (Dawes 1989).

[13] Apart from the informal sorting hypothesis presented here and the definition of types based on observed behavior in the first period, a sorting equilibrium can be constructed with fully optimizing players. However, the existence of a sorting equilibrium in the one-shot game and its properties critically depend on the subjects' beliefs about the definition and distribution of types and on the number of rights available to play B. Therefore we only sketch the equilibrium in this footnote, without formally testing it.

Suppose that conditional cooperators derive an extra utility of $\delta>150$ from both players cooperating (for a similar model, see Bolle and Ockenfels 1990 for mutual cooperation and Cooper et al. 1996 for unilateral cooperation), that subjects hold correct beliefs about the distribution of types and that the number of rights to play game B is larger than the number of cooperators in the group. Then, there exists a strictly positive price at which cooperative types pay the auction price and cooperate while egoists are indifferent between paying the price for B in order to exploit the cooperators and not paying anything to receive the non-cooperative payoff in game A for sure.

Assuming a constant percentage of cooperators of roughly one third, which is common knowledge, allows us to calculate at which auction price conditional cooperators are sorted from egoists. If two thirds of the subjects play game B and all conditional cooperators sort themselves into game B, then egoists are willing to bid $[EV(B)-EV(A)]_{Def} = 0.5*500 + 0.5*150 - 150 = 175$ cents. Cooperators are willing to bid $[EV(B)-EV(A)]_{Coop} = 0.5*(350+\delta) + 0.5*100 - 0= 225+0.5\delta$. Thus, any price between 175 and $225+0.5\delta$ induces sorting of player types (with the equilibrium price being 175), leading to higher cooperation rates in game B than in game A.

If the number of rights to play game B is smaller than the number of cooperators, the auction price must be such that conditional cooperators are indifferent between playing game B, meeting another cooperator with certainty and paying the price, or playing game A and meeting another cooperator only with a small probability. If one third of the group plays game B, then egoists are maximally willing to bid $[EV(B)-EV(A)]_{Def} = 500 - 150 = 350$ cents. Cooperators are willing to bid $[EV(B)-EV(A)]_{Coop} = 350+\delta$, i.e. the equilibrium price when there are more cooperators in the group than rights to play B is $350+\delta$.

*Hypothesis 1:*

(a) In period 1, nobody cooperates in game A but all B-players cooperate in the small B treatment and half of the B-players cooperate in the large B treatment.

(b) Independent of the size of group B, first-period cooperation rates in versions B are higher than in versions A of the game in the auction treatments. No such difference between versions A and versions B exists in the assigned treatment.

Subjects know the auction price and are informed about their individual earnings after each period, i.e. both money maximizers and conditional cooperators can learn and adapt their behavior accordingly. In small B-groups, complete sorting with only conditional cooperators playing game B is possible. In such a case, full cooperation could be sustained over time. In large B-groups and with only one third cooperators present, sorting can never be complete. Conditional cooperators whose counterpart defects in the current period stop cooperating, increasing the number of defecting subjects in the next period and thus resulting in a downward spiral over time.[14] Assuming that one third of the group are conditional cooperators, we predict for behavior over time:

*Hypothesis 2:*

(a) A subject who cooperates in the first period is more likely to cooperate again in later periods if his or her counterpart cooperated and less likely if his or her counterpart defected in period 1.

(b) Overall-cooperation rates are stable in the small B-auction treatments and decrease over time in the assigned and in the large B-auction treatments.

Finally, individual bids should reflect the expected value of playing version B rather than version A. In order to evaluate how much better it is to play version B rather than version A, a participant has to form beliefs about what strategy his or her counterpart will choose, consider that sorting may occur, and choose a strategy. In particular, every subject has to compute the expected payoff from playing B minus the expected payoff from

[14] A similar spiral has been observed in continuous-choice public goods environments where conditional cooperators' contributions do not quite match the average contributions of others, see Fehr and Schmidt

playing A given his or her strategy. If there are more rights to play game B than conditional cooperators present, the auction price in the first period is equal to the expected payoff difference between game A and game B for defectors. Conversely, if there are fewer rights to play B available than conditional cooperators present, the auction price is equal to the expected payoff difference for cooperators. Even if bids do not reflect the true expected value initially due to the complexity of the task, we expect them to converge towards it as subjects learn and update their beliefs about other players' behavior over time.

We derive point and comparative statics predictions for first-period behavior and for behavior over time:

*Hypothesis 3:*

(a) Auction prices reflect the expected value of playing game B minus the expected value of playing game A.

(b) First-period auction prices are higher, the larger the expected value of playing version B rather than version A is. In particular, auction prices are higher, the smaller group B is and the more periods of version B are played after the auction.[15]

(c) Over time, auction prices converge to the expected value of winning the auction.

## IV Experimental Results

In the following, the main findings are presented.

*Observation 1: First-period cooperation*

(a) Cooperation rates in version B of the game are close to 50 percent in all auction treatments.

In large B-groups, first-period cooperation rates are exactly (or close to) 50% in all auction sessions while in small B-groups, cooperation rates in none of the auction sessions

(1999).

[15] This prediction holds assuming that one third of the group are conditional cooperators, sorting takes place,

approach 100% (see table 3, Appendix B). The point prediction of hypothesis 1(a) is only partially supported.

(b) Cooperation rates are higher in version B than in version A of the game in the auction treatments but not in the control treatment.

First, note that there are no significant differences between the overall-cooperation rates in the control treatment and the eight auction sessions (Fisher's Exact tests, p>0.10).[16] This is consistent with the sorting hypothesis. At the same time, substantial differences between first-period cooperation rates in versions A and B can be observed in the auction treatments. For a graphic representation of cooperation rates in games A and B see figure 1 below. (Table 3, appendix B, reports the cooperation rate for each session separately.) In all auction treatments, but not in the control treatment, cooperation rates in versions B are significantly higher than in versions A. A Fisher's Exact test yields p=0.547 for treatment I, p=0.000 for treatment II, p=0.025 for treatment III, and p=0.046 for treatment IV.[17] Hypothesis 1(b) is supported.

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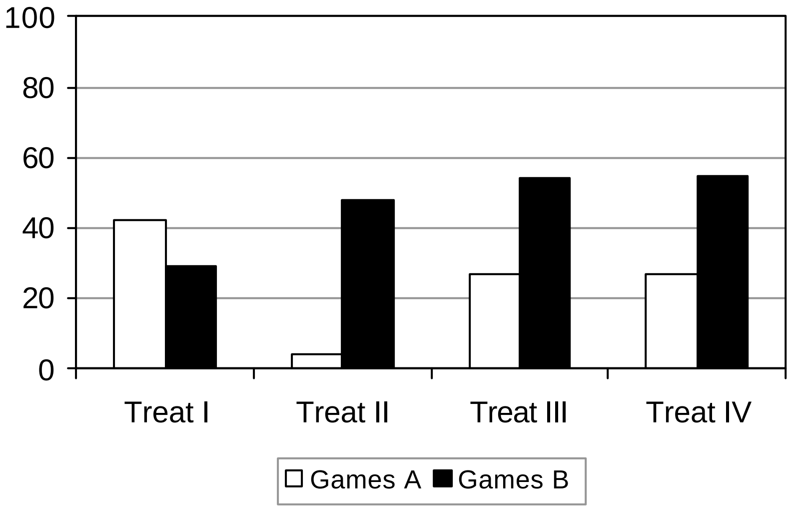
and both is anticipated by the subjects.

[16] Comparing the overall cooperation rate of each auction session with the cooperation rate in the assigned games, no significant differences can be found: A-1: p=1.00, A-2: p=0.431, A-3: p=1.000, A-4: p=0.805, A-5: p=0.167, A-6: p=0.805, A-7: p=0.626, A-8: p=0.799.

[17] At the level of individual auction sessions, the difference between versions A and version B is significant at a 10%-level in the three sessions of treatment I with large B and in one of the sessions of treatment IV: A-1: p=0.087, A-2: p=0.024, A-3: p=0.022, A-4: p=0.115, A-5: p=0.638, A-6: p=0.115, A-7: p=0.050, A-8: p=0.664. The data is pooled as there are no significant differences between the sessions in each treatment.

Figure 1: First-period cooperation rates in the control and the three auction treatments (in percent)




*Observation 2: Cooperation over time*

(a) There is evidence for both types of players, egoists and conditional cooperators.

In our experiments, 86 out of 252 subjects cooperated in the first period of the game. One out of the 86 continued to cooperate in all the remaining four periods. Most other first-period cooperators' behavior is contingent on their counterpart's type: 75 percent of the cooperators meeting another cooperator in the first period (N=32) are willing to cooperate at least once again in the future. On the other hand, only 24 percent of the cooperators meeting an egoist in the first period (N=54), are willing to ever cooperate again in the remaining four periods. Of the 112 money maximizers who meet another defector in the first period, 29 percent cooperate at least once in periods 2 to 5 (and 24 percent of the egoists meeting a cooperator in the first period (N=54) are willing to cooperate in later rounds). First-period cooperators thus do not behave differently in later periods than first-period defectors if their counterpart defects but are much more likely to cooperate again if their counterpart also cooperates (Fisher's Exact Test, $p<0.01$), supporting hypothesis 2(a). Table 4 in (appendix B) presents the results for the four different treatments.

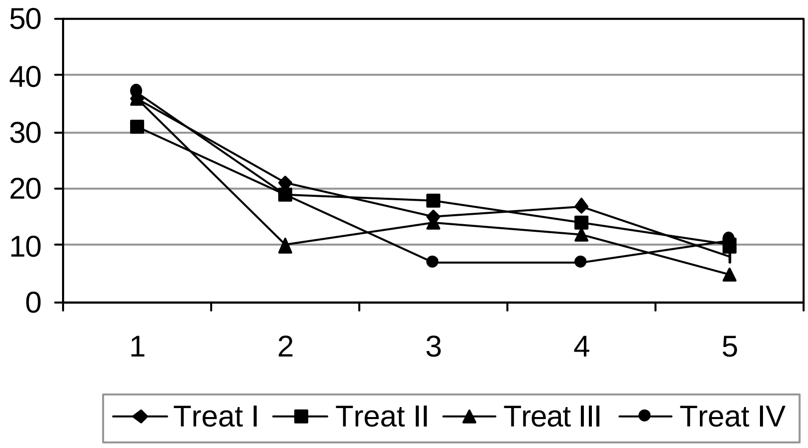
(b) Overall-cooperation rates in all treatments and in all games decrease over time.

The opportunity to self-select into groups of similar types does not prevent conditional cooperators from defecting in later rounds. Figure 2 presents the experimental data graphically.[18] The decrease in cooperation rates in the assigned treatment I is very similar to the decay in auction treatments II and III. The differences between the overall-cooperation rates in the control treatment I and the two auction treatments II and III are not significant in almost all periods ($chi^2$-tests: $p>0.1$ for all treatment comparisons in all periods, with the exception of Treat I-Treat III in period 2 where we find a marginally significant difference with $p=0.099$). Finally, no difference between treatments II and III can be observed ($chi^2$-tests: $p>0.1$). Incomplete sorting in both large and small B-groups induces a downward spiral. Hypothesis 2(b) is only partially supported.

Moreover, the decrease in cooperation is similarly strong in treatment IV with a single auction and in treatments II and III with an auction in each period. The differences between the overall cooperation rates in the three auction treatments are not significant in almost all periods ($chi^2$-tests: $p>0.1$ for all treatment comparisons in all periods, with the exception of Treat II-Treat IV in period 3 where we find a marginally significant difference with $p=0.083$). Thus, the decrease cannot be attributed to invaders, paying the price for B in later rounds in order to exploit the cooperators.

[18] For the data of the individual sessions, see table 3 in appendix B.

Figure 2: Overall-cooperation rates over time (in percent)




*Observation 3: Auction prices*

a) In the first period, auction prices do not correspond to the expected value of playing game B minus the expected value of playing game A.

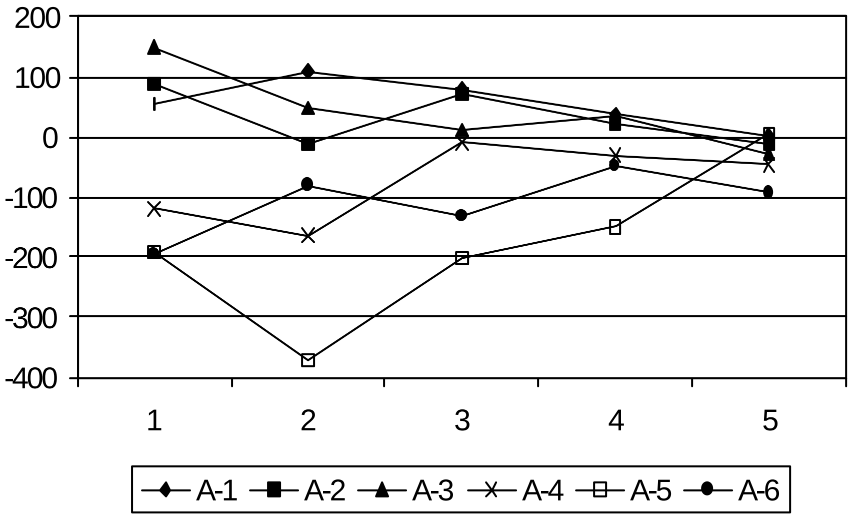
Most subjects bid more than zero. Out of 204 participants in the three different auction treatments, only 22 subjects bid zero cents in the first period. Everybody else, i.e. 85 percent of the participants, has some positive willingness to pay for the right to play version B rather than version A. First-period auction prices are higher than if subjects had not anticipated sorting at all. If no sorting was expected, defectors would bid 0 and cooperators maximally 66 cents in both auction treatments II and III (assuming that they expect one third of the group to cooperate). Table 5 (appendix B) shows that actual bids are much higher than these numbers. Hypothesis 3(a) is not supported.

If subjects had rationally anticipated the distribution of types, first-period auction prices should be just equal to the expected advantage of playing B instead of A. Computing the expected value of B over A by using the realized cooperation rates and subtracting the auction price for each session yields the graphs of figure 3.[19] It emerges that in the first

[19] For example, in the first round of A-1 the expected value of playing B instead of A for a defector is 0.5*500+0.5*150-(0.1*500+0.9*150)=140. Note that the expected values of defectors used in figure 3 are lower than the expected values of conditional cooperators who benefit from the "insurance" in game B. The auction price in treatment II (large B) should be equal to this expected value whereas in treatment III (small

period, the auction price is too low in treatment II (A-1 to A-3) whereas it is too high in treatment III (A-4 to A-6).

Figure 3: Expected value of playing B instead of A minus auction price (in cents)




b) Auction prices are higher for small than for large groups B, but mean bids do not differ significantly. When subjects can bid for the right to play a game for 5 periods rather than for 1 period, auction prices and bidding differs.

Table 6 (appendix B) reports the realized auction prices in all sessions. Bidding behavior seems very similar in treatments II (small B) and III (large B). A test comparing the means and the distribution of bids in the first period of treatments II and III reveals no significant difference between the two (Mann-Whitney test, p=0.18; Kolmogorov-Smirnov test; p=0.67). Assuming again that about one third of the group are conditional cooperators in both treatments, bids should differ as the value of playing small B (where everybody

B) the auction price can also be higher if there are more conditional cooperatores than rights to play B. Thus, part of the discrepancy in treatment III (A-4 to A-6) may be due to underestimating the expected values.

cooperates) is higher than the value of large B (where the probability to meet a cooperator is only one half). Hence, as bids do not differ, the price difference is due to the fact that the distribution of bids is truncated after a smaller number of bidders when B is small.

Comparing first-period bids of treatments II and III (where an auction takes place in every period) with the first-period bids of treatment IV (with only one auction in the first period) reveals that the means and the distributions of the bids significantly differ from each other (for II and IV: Mann-Whitney test, $p<0.001$; Kolmogorov-Smirnov test, $p<0.001$; for III and IV: Mann-Whitney test, $p<0.001$; Kolmogorov-Smirnov test, $p<0.001$). Thus, hypothesis 3(b) is not supported for the group size effect but it is supported for the period effect. Subjects seem to be able to figure out that bidding for the right to play game B for five periods is worth more than bidding for the right to play it for one period only. On the other hand, they do not bid more for smaller B groups than for large B groups.

(c) Auction prices and the differences in expected values between games B and A converge.

Figure 3 shows the difference between the expected value of playing game B and game A for defectors minus the auction price in each period, i.e. $[EV(B)-EV(A)]_{Def}$ - P.[20] In the three sessions of treatment II (large B), auction prices start out too low, whereas in treatment III (small B) auction prices are too high in the beginning. Over the five periods, as cooperation rates decrease, subjects adjust their bids to the decreasing true value of game B, supporting hypothesis 3(c).

## V Discussion

We find evidence for sorting in the beginning of our experiments, with higher cooperation rates in the insured version B than in the status quo version A of the game. No such differences can be found when versions A and B are assigned. While it has not yet been investigated how changes in the payoff structure affect sorting in the prisoner‘s dilemma, the few papers studying the impact of relative payoffs on cooperation rates in assigned prisoner's dilemma games support our results in the control treatment. Introducing an insurance mechanism in a two-person PD, which decreased the cost of unilateral cooperation from 40 cents to 10 cents, Ahn et al. (1998) did not find any effect on cooperation. Only if both the benefit of unilateral defection and the cost of unilateral cooperation were decreased from 40 to 10 cents, they found an increase in cooperation.[21]

Schotter (1998) tested the effect of an insurance mechanism in a profit sharing game with two equilibria, a high effort and a low effort equilibrium. Subjects' payoffs depended on their own effort levels, those of the other group members and on the group incentive formula. Two such formulas were compared: a high-vulnerability plan A and a low-vulnerability plan B in which a subject's payoff fell less steeply than under plan A for identical reductions in others' efforts. Thus, plan B compensated high effort workers to some degree, providing a similar insurance mechanism as was employed in our game B. The author found that subjects' behavior in plans A and B did not differ (unless they shared a common history of shirking).[22]

Similarly, in our experiment the provision of an insurance mechanism by itself does not lead to higher cooperation rates. Only when the right to play the insured version of the game is auctioned off, more cooperation is observed in the insured version of the game compared to the status quo version. This may not come as a surprise: Privileges in clubs are not just randomly given to people but sold and sometimes even auctioned off.

Still, to the best of our knowledge, nobody has found a sorting mechanism leading to stable cooperation rates over time yet. In Erhart and Keser's (1999) public goods experiment where subjects could leave their groups to form new ones, "cooperating subjects are on the run from less cooperative ones who follow them around." (p. 9) As

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[20] For the auction prices in the individual sessions, see table 6 in appendix B.

[21] Their average cooperation rate over all random matching treatments is also very close to ours, namely 31.6 percent. A change in payoff structures was first studied by Rapoport (1967) who called the relative cost of unilateral cooperation "fear" (payoff for mutual defection minus the payoff for unilateral cooperation) and the relative benefit of unilateral defection "greed" (payoff for unilateral defection minus payoff for mutual cooperation).

[22] While subjects could not choose between plans but were assigned to them, the author affects expectations about others' likelihood of choosing high effort levels using a different mechanism: Prior to playing the profit sharing game, subjects either participated in the minimum or the median coordination game. While subjects typically managed to coordinate on the payoff-dominant equilibrium in the median game (no shirking experience), they typically converged to the minimum in the minimum game (shirking experience).

cooperative subjects formed new groups, hoping to meet other cooperators, egoists constantly invaded and decreased cooperation rates over time. Offerman and Potters (2000) experimentally examined how auctioning off entry licenses (e.g. for oil drilling or airport slots) affects pricing behavior, also finding support for first-round sorting only. The most collusive players, setting the highest prices and earning the largest profits, self-selected into the market game in the first auction. However, in later auctions, no signs of such a selection effect were found. Sorting, thus, was not sustainable.

An experiment by Charness (2000) supports the relevance of sorting in a bargaining game. Subjects were sorted by the experimenter according to their offers in a first stage dictator game. In the second stage, people of the same 'type' were paired to play a bargaining game and informed of each other's type. Pairs of generous types bargained more efficiently than all other pairs. Even when subjects knew in advance that they would be sorted according to their allocation in the dictator game, this difference in bargaining outcomes was observed (albeit on a lower level). Knowing that one's partner is 'generous' or, as in our experiment, that she was willing to pay a price for the insured game, seems to confer useful information and to encourage 'friendly' behavior.

While the sorting explanation is consistent with most of our data, we want to explore an alternative hypothesis as well. Forward induction has been used to explain why auctions could lead to increased efficiency in coordination games. Van Huyck et al. (1993) and Cachon and Camerer (1996) allowed players of a coordination game with seven Pareto-ranked equilibria to opt out of the game. The price for the right to play the game served as an efficiency-enhancing coordination device if the price was high enough to exclude inefficient equilibria. While Van Huyck et al. (1993) argued that in line with forward induction, auction prices provide a means of tacit communication, Cachon and Camerer (1996) showed that 'better' equilibria were reached even if fees were imposed. This cannot be accounted for by forward induction as subjects could not choose whether to participate or not, but suggests that they acted according to the principle of 'loss-avoidance'.[23] Cooper et al. (1993) investigated the effect of one player getting an outside option in the battle-of-

[23] Similar to Cachon and Camerer (1996), Offerman and Potters (2000) report that "it is mainly the fact that an entry fee has to be paid rather than a selection effect that affects the outcome on which subjects coordinate." (p. 20).

the-sexes game. They also found only limited support for the relevance of signaling intentions about future behavior, i.e. forward induction, as players also react to dominated outside options.

Despite this mixed evidence regarding the explanatory power of forward induction, it could have played a role in our experiments if all individuals had been conditional cooperators. For conditional cooperators, the prisoner's dilemma is transformed into a coordination game. An auction price above 150 cents for game B helps such players to coordinate on the cooperative outcome in game B but not in game A. Differences in cooperation rates between versions A and B of the game could thus be accounted for. However, our data do not support such an explanation for two reasons: First, the overall-cooperation rate is insensitive to the size of group B. Forward induction would imply that with large B, more subjects cooperate than when B is small. Second, auction prices in all sessions of treatment II are too low (below 150 cents) for a forward induction argument to work. Although first-period auction prices in treatment III were sufficiently high for forward induction, they did not result in systematically different behavior than the low auction prices in treatment II.

## VI Conclusions

We have run an experiment that differs from past prisoner's dilemma studies in that our experimental subjects could choose between two PD payoff structures, the original game A and a modified version B in which the payoff from unilateral cooperation is increased. The right to participate in version B rather than in the original game A could be bought in an $n^{th}$-price, sealed-bid auction. The specific design was chosen for two reasons: First, we wanted to provide subjects with an "institutional choice" implying that they could decide which version of a game they wanted to play but could not just opt out. Taking the criticism seriously that game theory is of limited practical relevance because it does not allow for individuals to change the games they play (Brandenburger and Nalebuff 1996), we tested for the implications of transforming payoff structures in the laboratory. Secondly, the specific change in the payoff structure, the decrease in the cost of unilateral cooperation in game B, was chosen in order to reflect schemes used by various organizations to sort their employees, customers or insurees.

We compare various auction treatments with a control treatment in which versions A and B were assigned to the subjects. After the auction, significantly more subjects cooperate in the modified PD than in the status quo PD whereas there is no difference between cooperation rates if the two versions of the game were assigned to participants. Individuals willing to cooperate in period 1 thus self-select into the insured version B while defectors bid less and play version A. Such a segmentation of types may be especially interesting from an evolutionary point of view as it facilitates the "proliferation of nice traits" (Bowles 1998: 93).

First-period cooperators continue to cooperate if they have been paired with another cooperator in the first period. If their counterpart defected in the first period, the likelihood of ever cooperating again is as low for first-period cooperators as for first-period defectors, indicating that our subjects are either conditional cooperators or egoists. If sorting in the first period is incomplete, we should expect the dynamics of conditional cooperation to lead to a decrease of cooperation rates over time. This is what we observe: First-round cooperators who meet a defector in period 1 stop cooperating in later rounds. The decrease of cooperation rates makes the differences in expected values between playing version B and version A smaller and smaller. Auction prices reflect this trend. While we observe over- and underbidding in the beginning, the differences in expected values and the auction prices converge over time and are close to zero in the last period.

We find that auctioning off the right to play a prisoner's dilemma game provides a means of sorting in the beginning of the experiment but that sorting is not sustainable over time. More research remains to be done in order to better understand under which conditions sorting can be stabilized and cooperation among those who are more inclined towards cooperative outcomes can be maintained. Our experiments shed light on some aspects of such a mechanism.

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## Appendix A: Experimental instruction for auction treatments

## Welcome to this research project!

You are participating in a study in which you have the opportunity to earn cash. The actual amount of cash you will earn depends on your choices and the choices of the other persons in the study. At the end of the study, the amount of money you earned will be added to the show-up fee of $5. In addition to these instructions, you receive an envelope containing

- a Code Number Form
- a Practice Form
- a Private Valuation Form
- an Auction Form marked with your code number
- a Decision Form marked with your code number

### What the study is about:

The study is on how people decide. You have to make two decisions:

***(i) How much do you want to bid for the right to participate in a specific situation?***

The right to participate in a specific situation will be auctioned off. Situations A and B, presented below, only differ in the size of the payoffs. You will be asked to privately submit your bid for the right to participate in situation B instead of in situation A. "Participating in situation B" means that you will play according to payoff table B for 5 rounds, with new partners in each round. If you remain in situation A, you will play according to payoff table A for 5 rounds, with new partners in each round.

The top n bidders will participate in situation B. These n bidders will have to pay the amount that equals the highest rejected bid, i.e. the amount that the $n+1^{th}$ bidder offered. The other m bidders who did not bid high enough will participate in situation A. <u>The values for n and m are indicated on the blackboard</u>.

***(ii) Which alternative do you choose in the situation you participate in?***

You and another person who is in the same situation as you are have to choose between two alternatives, X and Y. The payoff table tells you how much money you earn depending on what you choose and what the other person chooses.

### How the study is conducted:

The study is conducted anonymously and repeated five rounds. Participants are only identified by "code numbers". In order to guarantee privacy and anonymity, <u>do not show anyone your code number</u>! You are randomly matched with another person in each round. Before beginning with the actual research project, we will run one practice auction to familiarize you with auctions.

**START**

**I. Practice: Auction for a hypothetical good:**

In this auction, 3 identical hypothetical goods are auctioned off. In your envelope you received your private valuation of the good. This is the amount of money the good is actually worth to you. Each participant may only bid for one good. Thus, the 3 highest bidders will each receive one good. The 3 highest bidders will pay the amount that the 4th highest bidder offered. Everybody else will neither receive nor pay anything.

Please write the amount you want to bid on the "Practice Form" and put it into the box. We will collect all forms and determine the 3 winners.

> If a tie occurs (for example, the 3rd and the 4th bids are equally high), a random device will be used to determine who will receive the good. The price to be paid is the next highest bid, in this example, it would be the 5th highest bid.

In order to demonstrate how the auction works, we will write down all bids on the blackboard and explain how the price is determined. This is only done in the practice auction, not in the following real auction. You don't have to pay anything in the practice auction.

## II. Research Project:

Situations A and B are represented by the following payoff tables. Tables A (B) read as follows:
If you and the other person choose Y, each of you earns 150 cents.
If one of the two of you chooses Y and one of the two of you chooses X, choosing Y earns 500 cents and choosing X earns 0 cents (100 cents).
If you and the other person choose X, each of you earns 350 cents.

**Payoff Table A**

| Number of Persons choosing X | Outcome for X (cents) | number of persons choosing Y | outcome for Y (cents) |
|---|---|---|---|
| 0 | -- | 2 | 150 |
| 1 | 0 | 1 | 500 |
| 2 | 350 | 0 | -- |

**Payoff Table B**

| Number of Persons choosing X | Outcome for X (cents) | number of persons choosing Y | outcome for Y (cents) |
|---|---|---|---|
| 0 | -- | 2 | 150 |
| 1 | 100 | 1 | 500 |
| 2 | 350 | 0 | -- |

**Procedure:**
We first run the auction and determine whether you are in situation A or B. You and everyone else will remain in the same situation during the whole study. Once you know whether you are in situation A or B, you will have to decide between X and Y. You will be randomly matched with a new person in each round. This procedure is repeated five times.

**Round 1:**
We now auction off n rights to participate in situation B in the next round. The n highest bidders will each participate in situation B and pay the amount that the $n+1^{th}$ bidder offered. The m other bidders will participate in situation A and pay nothing.

> If a tie occurs (for example, the $n^{th}$ and the $n+1^{th}$ bids are equally high), a random device will be used to determine who will be in situation B. The price to be paid is the next highest bid, in this example, it would be the $n+2^{th}$ highest bid.

Please write on the Auction Form how much you want to bid in the auction, put it into the envelope and then into the box which is passed around. We will privately inform you whether you are among the n highest bidders in this auction or not.

After the auction, you know in which situation, A or B, you are. You are randomly paired with a person who is in the same situation as you are.

Please carefully read the corresponding payoff table, A or B, before making a choice. Indicate the situation you are in, A or B, and your choice for Round 1, X or Y, on the Decision Form, put it back into the envelope and then into the box which we will pass around.
End of round 1.

We will now determine your earnings according to your choice and the choice of the other person, and privately inform each of you how much money you earned in this round. For this purpose, we will again pass the box around. Please take the envelope marked with your code number out of the box. It contains the Decision Form now also indicating your earnings. Do not tell or show anybody else your result.

**Round 2:**
You remain in the same situation as in round 1. You are randomly matched with a new person who is in the same situation as you are. Please again choose between X and Y from "your" payoff table. Indicate your choice for 'Round 2' on the decision form, put it into the envelope and then into the box which we will pass around. We will compute your earnings again and privately inform each of you how much money you earned in this round.
End of round 2.

**Rounds 3-5:**
The exact same procedure as in the previous round will be repeated.

END OF THIS STUDY.
Please put the Decision Form back into the envelope and then into the box. Keep your Code Number Form! You will have to present your Code Number Form in order to receive your earnings.
If you have any questions, please address them to Iris_Bohnet@Harvard.edu

Thank you for participating in the study. Iris Bohnet

**Appendix B**

Table 3: Cooperation rates in all periods

| **Periods** | | **1** | **2** | **3** | **4** | **5** |
|---|---|---|---|---|---|---|
| Assig. I | A+B | 36% | 27% | 15% | 17% | 8% |
| | A | 42% | 21% | 21% | 13% | 8% |
| | B | 29% | 33% | 8% | 21% | 8% |
| Auct. II-1 | A+B | 35% | 31% | 23% | 27% | 15% |
| | A | 10% | 0% | 0% | 10% | 0% |
| | B | 50% | 50% | 38% | 38% | 25% |
| Auct. II-2 | A+B | 25% | 13% | 13% | 4% | 8% |
| | A | 0% | 10% | 0% | 0% | 10% |
| | B | 43% | 14% | 21% | 7% | 7% |
| Auct. II-3 | A+B | 32% | 14% | 18% | 9% | 5% |
| | A | 0% | 0% | 13% | 0% | 0% |
| | B | 50% | 21% | 21% | 14% | 13% |
| Auct. III-4 | A+B | 30% | 13% | 23% | 10% | 3% |
| | A | 20% | 5% | 10% | 5% | 0% |
| | B | 50% | 30% | 50% | 20% | 10% |
| Auct. III-5 | A+B | 56% | 11% | 11% | 17% | 11% |
| | A | 50% | 17% | 8% | 17% | 0% |
| | B | 67% | 0% | 17% | 17% | 33% |
| Auct. III-6 | A+B | 30% | 7% | 7% | 10% | 3% |
| | A | 20% | 0% | 5% | 5% | 0% |
| | B | 50% | 20% | 10% | 20% | 5% |
| Auct. IV-7 | A+B | 43% | 18% | 11% | 7% | 14% |
| | A | 28% | 6% | 6% | 6% | 11% |
| | B | 70% | 40% | 20% | 10% | 20% |
| Auct. IV-8 | A+B | 31% | 19% | 4% | 8% | 8% |
| | A | 25% | 19% | 6% | 0% | 0% |
| | B | 40% | 20% | 0% | 20% | 20% |

Table 4: Players' first-period contingent behavior over time

| Treatments | Period 1: Players' Experiences | Periods 2-5: Share of players who cooperate at least once (or more) |
|---|---|---|
| I: Assigned games | C meets C (N=8) | 75% |
| | C meets D (N=9) | 33% |
| | D meets D (N=22) | 41% |
| | D meets C (N=9) | 22% |
| II: Multiple large B-auctions | C meets C (N=8) | 63% |
| | C meets D (N=14) | 43% |
| | D meets D (N=36) | 25% |
| | D meets C (N=14) | 29% |
| III: Multiple small B-auctions | C meets C (N=10) | 70% |
| | C meets D (N=18) | 17% |
| | D meets D (N=32) | 41% |
| | D meets C (N=18) | 28% |
| IV: Single small B-auction | C meets C (N=6) | 100% |
| | C meets D (N=13) | 8% |
| | D meets D (N=22) | 5% |
| | D meets C (N=13) | 15% |

Table 5: First-period average bids (in cents)

| Treatments | Egoists' bids | Cooperators' bids |
|---|---|---|
| Treatment II (large B) | 110 (N=50) | 236 (N=22) |
| Treatment III (small B) | 157 (N=50) | 295 (N=28) |

Table 6: Auction prices in all periods (in cents)

| **Periods** | **1** | **2** | **3** | **4** | **5** |
|---|---|---|---|---|---|
| Auction II-1 | 85 | 66 | 68 | 76 | 84 |
| Auction II-2 | 62 | 25 | 2 | 2 | 0 |
| Auction II-3 | 25 | 26 | 20 | 15 | 15 |
| Auction III-4 | 225 | 250 | 150 | 82 | 81 |
| Auction III-5 | 250 | 275 | 233 | 150 | 110 |
| Auction III-6 | 300 | 150 | 150 | 100 | 110 |
| Auction IV-7 | 450 | - | - | - | - |
| Auction IV-8 | 350 | - | - | - | - |

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